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NATIONAL INSTITUTE FOR MEDICAL RESEARCH,
THE RIDGEWAY, MILL HILL,
LONDON, N.W.7.

26th October, 1960.

Professor J. Lederberg,
Dept. of Genetics,
Stanford University,
Stanford,
California.

Dear Josh,

Thank you for your two letters of 3rd and 10th October. I am keeping the typescript of the periodate paper as you suggest and have only a few comments (see the end).

The other points.

1. Yes, the symposium on space travel is to be published as a small book by the Institute of Biology, late this year. If I get any reprints I will send you one. An examination of some soils from roots of old herbarium plants (Kew Herbarium) gathered before 1863, shows much highers counts of sporing bacilli than controls (like the paper, the flowers, etc) and I hope to write this up briefly somewhere. The "half life" of these soil bacteria seems around 20 years under such conditions. Identification of them is proving troublesome.

2. This puts a slightly new light on the plan of Paul Ehrlich and myself to look at the grasshopper glacier, since we would like them to be several centuries old, and the evidence (such as it is) is that they are very recent. Still, I will re-draft an estimate, and seek permission from the M.R.C. for such a project.

3. I have cited your paper as you suggest, omitting the Nice Conference volume.

4. Yes, not much more can be done without a male-substance assay, it seems.

5. As to the Mohole project, I will be glad to help if I can. It may prove very interesting.

6. As far as I know, there were no experiments to show that addition of periodate after pairing did not reduce the expected recombinant count. That a number of matings did continue was shown in experiments SN 7,12, 64 but my data give little clue as to the significance (since the non-periodated controls

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were allowed to continue mating; they should be diluted to prevent further mating). Yet the numbers of recombinants was in the range of 10-20% of the controls after allowing for the killing by periodate. This seems too high to explain the periodate effect, which often gives only 1% of expected matings. The additional recombinants could plausibly be due to later matings.

7. Specific corrections on periodate paper:

p. 1. Will you add the references? (they were omitted from my copy)

p.1, genotypes; W 3064 V^r should be $V^r_{\lambda 2}$?

W 6 delete S

W 3876 Sm^r to be S

W 3776 delete Sm^s

W 3770 Sm^r to be S

Table 1. The viable counts are not clear, since the female counts are omitted (they were from L to R, 98, 120, 148, 120). Also add 'male' and 'female' on appropriate line.

Table 2. is "Untreated male (mixture plated immediately)" sufficiently clear, or would it be best amplified?

Discussion. Yes, a note on the acrosome would be good.

8. We will be glad to pay for our 150 reprints if you wish: there will be no difficulty if we get an invoice.

I will not hold this up with other points, but sometime I would like to know your frank views on the grasshoppers: - is it worth a great effort?- will it be in well with exobiology? Joan joins with me in sending our warmest wishes to you and Esther, and to all in the department.

Yours ever,

Sydney